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ABSTRACT

This paper discussed Consortium for Longitudinal Studies findings which demonstrate that, contrary to first impressions from earlier evaluation studies, early intervention preschool programs do have generally positive long term effects. A public information campaign to communicate these findings through all media is advocated. Problems of clarifying the picture of preschool programs, such as variety in program quality, are discussed. To standardize programs for assessment purposes, minimum criteria or experience thresholds for legitimate programs of preschool education should be identified. For more precise program assessment, organismic variables such as hemispheric dominance, cognitive style, anxiety-neuroticism, extraversion and self esteem are considered more relevant to differential response to treatment questions than demographic variables and gross classifications such as pretest IQ, family structure and maternal educational expectations for the child. Further investigation of background variables related to children who are "super-responders" and "hard core unresponsives" to early education is advised. It is suggested that encouraging but limited data on reliability of findings should be tested by comparison with data from other high quality programs. (RH)

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Reflections on the Long Term Effects of Early Intervention

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These comments are organized roughly into two categories or sets editorial and analytical. Both sets are based upon a study of the 1978 and 1979 Consortium Reports* and deal more with the general, overall picture than with individual programs.

First, the editorial. From the outset of the preschool intervention movement, doubting Thomases have continued to remind us that whatever any positive early outcome of early education, the acid test is long-term impact. Paradoxically, evaluations of first grade school programs, or third grade, or seventh grade, or any regular grade (except preschool) have never been subject to stringent criteria of demonstrated, specific long-term effect. Yet one or two year programs of preschool experience somehow are cast unwaveringly into this light. Thus we are extremely fortunate, I think, that certain "keepers of the faith" have persevered through trying times and circumstances to provide us with invaluable data. To persevere, however, has been to court risk. The spectre of uncertainty about impact has loomed strongly through the years. Data could chill as easily as warm those of us committed to early education. From these follow-up reports,

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* See references

however, it is increasingly clear that the data are warming. Particularly impressive are indices of the social validity of preschool programs, exemplified by David Weikart's Ypsilanti data. Unprecedented cost-benefit data further buttress the case for early intervention in strikingly pragmatic terms.

Now that a generally positive longer-term picture is coming into sharp focus, what are we going to do about it? If we can accept with confidence the evidence now available, a renewed effort for massive dissemination seems in order. Though Consortium members and staff have been at work along these lines, longitudinal data should be communicated to the public, including state and federal legislators. The reason for this, I think, is that our society has perhaps never recovered from somewhat negative "first impression" reports such as Westinghouse-Ohio University. A campaign to communicate longitudinal data would include all media, especially television (as, say, 60 Minutes, Donahue, and the Tonite Show) and widely read magazines (as Time, Newsweek, U.S. News and World Report, Readers Digest, and Family Circle) the task is one of attitude change toward increased public support for carefully articulated preschool education. Meanwhile, each of us should be sure that our students and colleagues are informed about the Consortium and its work.

Of course, important questions remain, such as why programs seem to work. Paradoxically, and insofar as I know, no one knows why aspirin works. Yet aspirin-taking continues undaunted, a policy without which modern society might collapse. We do have some promising hypotheses about why preschool education

can work. These need further study which is part and parcel of sustained financial support for early education in evaluation research. I suggest here, however, that we expand the scope of such research genuinely to explore what is possible for early intervention. This means a no-holds barred, pulling out of all stops approach. We must employ whatever is necessary to design and implement the highest quality experimental preschool program for large numbers of children. Such a program could be created by experienced professionals such as the present Consortium members and simultaneously replicated in the four major geographic quarters of the nation. In other words, the favorable longer-term results about early education reported here indicate what can occur but always under less than the best of conditions. Based on the Consortium experience, how much more might be accomplished with unlimited financial and moral support? Should we not storm HEW with a proposal to give preschool education the fairest of all possible tests? One basic objective of such a test could be to identify minimum criteria, or experience thresholds, necessary for any legitimate program of preschool education in the United States. Strong cues for this already exist (e.g., Bronfenbrenner, 1974). But even these have emanated from limited contexts. Moreover, the past research has been delineated by fairly narrow measurement concerns, as IQ performance. Broader social competence criteria clearly hold greater promise for long-term perspectives.

Having attempted an impassioned plea for action to support further Consortium work, allow me to comment upon the data in more conventional analytical terms. First, I fear that certain

conclusions in the 1978 report may be overstated or misleading. For example, it has been concluded that quality preschool experience benefits all kinds of low income children, with no data to suggest that some kinds of children benefit more than others. The hazard is taking this conclusion to mean that all kinds of children benefit equally. Benefit, of course, is a matter of degree, and data still are insufficient to rule out possible trait x treatment interaction effects. The "child characteristics" variables thus far examined are either demographic or reflect gross classification criteria: pretest IQ, sex, race, birth order, maternal educational level, family size, family structure (father absence-presence), and maternal educational expectations for child. More distinctly relevant to a "differential response to treatment" question are certain organismic variables such as hemispheric dominance, cognitive style, anxiety-neuroticism, extraversion, and self esteem. Certainly such variables should somehow be accounted for before generalizing too widely about a lack of interaction. And still other social variables, such as peer relations and patterns of parental authority, may be important. In short, it seems presumptious to suggest to the public that no matter what kind of a program in which a given child may participate, the net effect will be the same.

Obviously, more thoroughgoing interaction analyses are not possible from existing Consortium data; the range of initial and follow-up assessments has been understandably limited. But it may behoove us to consider further post hoc strategies better to understand possible differential response phenomena

(see Shulman, 1970). There is variation in across measures within experimental groups in all Consortium programs. The question is why? If we can rule out the "child characteristic" variables already examined, additional investigation of background variables in these low income children may be helpful. This could involve a comparative analysis of experimental children who have shown the most and least progress over time subsequent to their preschool experience--the "super-responders" versus the "hard core unresponsives." It may turn out that factors associated with long term outcomes enter the picture after the preschool years. Then again it may not. The point is that a fuller understanding and prediction of intervention effects will require systematic study of sources of variation in program outcomes.

Another underlying concern is replicability or reliability of findings. Thus far, evidence for reliability is encouraging. But the data are limited to a relatively small number of extraordinarily carefully developed and implemented programs. The 1978 Report indicates that all programs have produced similar results, particularly in relation to reducing the incidence of children's assignments to special education classrooms. The two common ingredients in these programs are overall high quality and competent delivery of services. Suppose that we include still other competently implemented, high quality preschool programs not in the present inner sanctum? A possible source of information is the Head Start data pool. A sampling of highest quality Head Start Center programs from the 1965-70 period could lead to an identification of subjects for comparative

long-term impact assessment as in the Consortium study. If similar results obtain, then the argument for preschool intervention should be further strengthened. If not, possible important differences between Consorti programs and generic Head Start programming would be at issue. In either case, we stand to gain in our rational inquiry about early education.

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